TRACTS

Written by the Honourable

Robert Boyle.

Fellow of the R. Society.

OF

A Diſcovery of the Admirable RAREFACTION *of the* AIR.

New Obſervations about the DURATION *of the* SPRING *of the* AIR.

New Experiments touching the CONDENSATION *of the* AIR *by meer* COLD; *and its* COMPRESSION *without Mechanical Engins.*

The admirably DIFFERING EXTENSION *of the ſame Quantity of* AIR *rarefied and compreſſed.*

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Advertiſement.

THe Author *of the following* Papers *ſuppoſeth his* Readers *to have learned*, *either from the* Books *he hath publiſhed*, *or from what hath been borrowed thence by other* Writers, *the Structure and more familiar Uſes of a* Pneumatical Engin *of his*, *mentioned by ſeveral* Authors *under the Name of* Machina Boyliana; *with whoſe Deſcription therefore thoſe are deſired to acquaint themſelves, that ſhall think it worth the while to underſtand as well as read the following* Papers; *About which it might be further taken notice* *of, that the firſt of them was indeed written to a* Learned Friend *(though his Name be not now annexed for certain reaſons;) preſently after which the Three others were thought fit to be ſubjoyned. As for the omiting of the Complements and Forms uſual at the cloſe of Epiſtles, the* Author *did it as well to ſpare the* Reader *as himſelf; who hopes he may be excuſed, if the Tranſitions from one Diſcourſe to another, and even the Style and Method of them, be not ſo ſmooth and regular, in regard the enſuing Writings were traced, when he was afflicted with a great fit of Sickneſs, that kept him from ſo much as once reading over himſelf, what he had indited.*

ERRATA.

P. 2. l. 25. r. *Air* for *Fire.* p. 9. l. 26. r. *great gap.* p. 13. l. 18. r. *outward* for *utmoſt.* p. 19. l. 13. r. *laid it round.* p. 23. l. 1. r. *our* for *or.*

A DISCOVERY
Of the Admirable RAREFACTION
OF
AIR
(even without Heat)
Imparted in a LETTER to a FRIEND.

DO not imagine, Sir, that I did at all wonder to ſee you yeſternight ſo much admire, to hear me talk with ſo much ſeeming Extravagancy about the *Rarefaction and Condenſation of the Air*; for I confeſs, that I did deliver ſomething on that occaſion, that might eaſily at firſt ſight appear ſo near impoſſible as to be utterly improbable.

And though you were pleas'd even on ſuch an occaſion to expreſs a very favorable Opinion of my Veracity, yet thinking it fit, that ſuch an Obligation ſhould not divert but engage me, to endeavor to juſtifie you to your ſelf, by confirming what I ſaid to you; I have already ſought and found among Papers, many years ſince laid aſide, ſome that will enable me to make good more than what the diffidence of my Memory allow'd me to ſay in the very boldeſt part of my yeſternights Diſcourſe: For now that I luckily find not only the Originals of the Relations whereof this Paper contains Copies, but that my Engine is in good order; I am ſo far qualified to countenance a Diſcourſe, wherein I kept ſomewhat within compaſs, that though it will perhaps coſt me much pains and trouble to make *ex tempore* Experiments fully equal to the encloſ'd; yet if any juſt doubt ſhould require it, I preſume I can make Ocular Proof, of at leaſt as much as I laſt night told you.

And now 'tis time, after having, contrary to my Cuſtom, rais'd in you a high expectation, that I endeavor in ſome meaſure to anſwer it, which I hope I ſhall the more eaſily do, becauſe the agreement, you have often had occaſion to obſerve between the Relations regiſtred in my *Adverſaria*, and the *Phænomena* of the Experiments they deſcribe, will, I preſume, make it needleſs to perſwade you that the enſuing Trials, being tranſcribed thence, may be ſafely credited. Wherefore I ſhall proceed to annex them, as ſoon as I have premiſed a few Hiſtorical Lines by way of Manuduction to them.

'Tis now many years ſince, that having a deſire to reduce the Fire to a degree of Rarefaction that appear'd to be conſiderable, upon ſurer grounds than ſlight Conjectures, I attempted to do it by the help of Heat, and particularly by that of an *Æolipile*, which I have mentioned in another Tract; but finding that the diligent *Merſennus* had, if there be no miſtake in his account, been able to rarifie Air that way, full as much or more than I could, I betook me to try, whether I could not by the Spring of the Air (without Heat) procure a greater expanſion of it? I found (as I have long ſince elſewhere related) that in the Pneumatical Engin, which has been ſince called the *Machina Boyliana*, I could encreaſe the

New Phyſico-Mechanical Experiments. Exper. 6.

Ibidem.

the Expanſion of the Air, till the Body attain'd to about 152 times its former and uſual Dimenſions. But this Expanſion, though it were above twice as great as the utmoſt procured by *Merſennus*, did not yet ſatisfie me, but put me (according to what I there intimate) upon another Contrivance; which though put in practice eight or nine years ago, (as the date of one of the Trials may inform you,) had the relation of its Succeſſes laid aſide among thoſe of others, made in the ſame Engin which yet lie by me unpubliſh'd. So that I may now proceed to give you the Tranſcripts of the Trials themſelves, as they were haſtily and inelegantly, but very faithfully, ſet down among my *Pneumatical Collections*. And this I am ready to do, as ſoon as I ſhall have intimated to you, that in that Noble Collection of Experiments that has about two years ſince appeared in Publick, as the Firſt-fruits of the juſtly famous *Florentine Academy*, I find, that thoſe *Virtuoſi* had according to their ſagacity, ſo advanc'd the Extent of the Air, as without the help of Heat to bring the Dilatation to exceed 173 times its former Dimenſions; and that which made their Improvement the more conſiderable, and conſequently the more worthy of them, is, that they procured this great Rarefaction, as well as I had done mine, by the Air's own Spring; and had ſurpaſs'd without the help of my Engin, what I was then at firſt able to do by the Conveniences that it afforded me. Whereupon remembring what I had perform'd in that kind ſeveral years before, I ſought among my Papers for the Trials I had then made, and found thoſe Notes, whereof I now at length think it high time to give you the promis'd Copies in the following Terms.

Experiment I.

WE took a round Glaſs-Egg (as they call them) of clear mettal and furniſhed with a Pipe or Shank of ſome Inches in length; this we fill'd with water, and convey'd both it, and a Viol with Water in it, into a Receiver of a convenient ſize, and by pumping the Air out of it, we made the Bubbles both in the Egg and the Viol to diſcloſe themſelves in great numbers; ſo as to make the Liquor in the Glaſs-Egg ſeem to boil, and to make all that was in the Shank really to run over. When we thought the Water was ſufficiently freed from Air, which it was not quickly brought to be, we took out the Glaſſes and fill'd up the Pipe of the Egg with Water taken out of the Viol, and inverted it into more of the ſame Water, in ſuch manner that the Egg was quite full, Shank and all, excepting a ſmall Bubble of Air that we purpoſely left to gain the top of the Egg; where the Glaſs being tranſparent, with a pair of Compaſſes we meaſured as accurately as we could, and found it to be a Tenth and leſs than two Centeſms of an Inch. Then putting the Glaſſes again into the Receiver, we ſet the Pump a work, and the little Bubble after a while began to expand it ſelf, which when it had once done, it did at each ſuck ſtrangely increaſe, till at length it drove all the Water out of the round part of the Glaſs. And leaſt it might be objected, That 'twas only the ſubſiding of the Water upon the with-drawing of the outward Air, that before kept it up to the Top of the Glaſs, we caus'd the pumping to be ſo continued, till the expanded Air had ſeveral times driven the Water in the Pipe of the Egg a pretty way beneath the level of the external and ſurrounding Water in the other Glaſs. This done, we let in the Air by degrees, with a deſign to obſerve, what Bubble we ſhould find at the Top of the Egg, when the Water ſhould be again driven up into its Cavity. But the expanded Air had forced over ſo much Water, that there remain'd not enough to fill the Globulous part of the Egge: Wherefore we tried the Experiment again, and when we had proceeded thus far, we compar'd the above-mention'd Diameter of the ſmall Bubble, with that of the Spherical part of the Glaſs, which we took with a pair of *Callaper Compaſſes*; and though we found it to be ſomewhat more than 20 times as great, yet being willing rather to diſfavour than flatter the Experiment, we ſuppos'd the two Diameters to be as 1 to 20, and conſequently, ſince as *Euclid* demonſtrates, the Proportion between Spheres is triplicate to that of their Diameters, and in our Caſe the Cube of the leſſer Diameter being 1, is alſo but 1, the Cube of 20 the greater Diameter muſt be 8000; and ſo the Air appears to have, by expanding it ſelf, acquir'd a place 8000 times as big as it poſſeſt before. Nor was it overſeen by us, that the Globulous part of ſuch Glaſſes as we us'd is ſcarce ever made Spherical. But not only I, but Dr. *Wallis*, who was pleas'd to aſſiſt at the Experiment, concluded, that the Cavity of the Shank, which the expanded Air drove the Water from, but which we did not compute, would make abundant Compenſation for the two above-mentioned particulars. After this, for further ſatisfaction, we took water, laboriouſly freed from Air, and putting it into the ſame Glaſs-Egg, we inverted it as before, but left not any Bubble in it. This we did, that, in caſe we could make the Water ſubſide, the Experiment might prevent a ſuſpicion, that ſome Air latitant in the Water might increaſe the Bubble that was formerly left in it; having then exhauſted the Receiver as much as before, and, if we miſtook not, more, the Water in the Egg did not at all ſubſide; but at length, with obſtinat pumping, a Bubble diſclos'd it ſelf, and drove all the Water clear out of the round part of the Glaſs; and, though by reaſon of ſome ſmall Leaks that we could

El. 12. *Propoſ. ult.*

not find or ſtop, we were not able, as before, to make the expanded Air depreſs the Water in the Shank beneath the ſurface of the external Water, yet we wanted very little of it; and then out of wearineſs giving over, we found, that when the Water was impell'd up again into the Egg, there was at the top of it a Bubble, whoſe Diameter we meaſur'd as faithfully as we could, and found it to be to the Diameter of the Globular part of the Glaſs as 1 to 14; ſo that, though the little Bubble had been a perfect Sphere, yet Spheres being, as was lately noted, in triplicate proportions to their Diameters, the Bubble when expanded, muſt have been 2744 times as big as the Bubble unexpanded. But Dr. *Wallis*, who will be allowed to be a very competent Judge in theſe matters, obſerving (what we all took notice of) the great thinneſs of the Bubble, poſitively and conſtantly affirm'd, that he could not eſtimate it to be at moſt any bigger than the third part of a perfect Sphere of that Diameter; by which eſtimate the expanſion of the Bubble muſt have reach'd to 8232 times its natural Dimenſions.

NB. By letting in Water into the Receiver as much as it would admit, we found, that, by reaſon of ſome ſecret Leak, we had not been able ſo to exhauſt it, but that there remain'd ſome Air.

Experiment II.

June 2. 1662. A Small and almoſt inconſpicuous Bubble expanded it ſelf, when the ambient Air was pretty well exhauſted, to more than 10000 times its former extent. The manner thus; We took a ſmall Bolt-head, blown by a Lamp, which contain'd in all about 80 Grains of Water, and inverting the ſmall Neck into a Jar of Water, it was included in the Receiver, and the ambient Air being exhauſted, ſtore of Bubbles roſe out of the Water, and expanding it ſelf, quickly drove all the Water out of the Bolt-head. Then re-admitting the outward Air, the Bolt-head was preſently almoſt fill'd, and all the expanded Air ſhrunk into a Bubble little bigger than a ſmall Pins head; then taking the Bolt-head out of the Water, and inverting it, that the Bubble might get out at the Neck, we carefully fill'd it up with the Water that had been freed from Air, and then inverting it as before into the Jar with Water, we again included it, and, after ſome exuctions, found, that there was gotten out of the Water into the Neck a very conſpicuous Bubble, which upon the admitting of the Air, ſhrunk almoſt into an inviſible one, and aſcended into the head of the Glaſs. Then again exhauſting the Receiver very well, we found it expand it ſelf ſo as to fill all the Capacity of the Bolt-head, and to drive out almoſt all the Water. And upon the re-admitting of the Air, it again ſhrunk into a Buble whoſe Diameter (according to our beſt eſtimate) was not bigger than one two and twentieth part of the Diameter of the head of the above-mention'd Glaſs; ſo that to fill the whole Cavity of the Head only, it expanded it ſelf 10648 times: but becauſe it fill'd likewiſe the greateſt part of the Neck, we found by weighing the Water that fill'd that part, and the Water that fill'd the Head, that the Capacity of that part of the Neck, was almoſt a third of the Capacity of the Head, being as 141 to 481: if therefore 481, the Capacity of the Head, contain'd it 10648 times; 141, the Capacity of the Neck, muſt contain it $3121\frac{167}{481}$ times; ſo that, in all, the ſmall Bubble of Air was expanded to above 13769 times its former Bulke.

The Diameter of the ſmall Bubble retracted was $\frac{1}{37}$ of an Inch.

The Diameter of the out-ſide of the Head of the Glaſs was $\frac{29}{38}$ of an Inch.

The Water that fill'd the Head only weigh'd $60\frac{1}{2}$ Grains.

The Water that fill'd the Head and as much of the Neck as the Air had before expanded it ſelf into, weighed 78$\frac{1}{8}$ Grains; ſo that that part of the Neck weigh'd 17$\frac{5}{8}$ Grains.

The Bolt-head it ſelf weigh'd 15 Grains.

I might have ſet down this Second Experiment un-accompanied either with the Firſt, or with that I am going to ſubjoyn; becauſe the expanſion produced by neither of them was, at leaſt by meaſure, ſo vaſt, as that produc'd by the Trial newly mention'd: but this was ſo ſtupendious, that I thought it not ſo fit to preſent it to you by it ſelf alone, but rather accompani'd with other Experiments, the leaſt proſperous of which produc'd a Dilatation of Air ſufficient for my preſent purpoſe, and ſuch as may not a little confirm, That what is recited in the Second Experiment, was neither a lucky chance, or miſtake. And that may be enough for my preſent purpoſe; for as for the little Abatements, that ſome will perhaps think fit to be made upon the ſcore of the unequal thickneſs of Glaſs or ſome ſuch Circumſtances, they are not conſiderable enough to deſerve to be now ſolicitouſly debated, nor to hinder the expanſion that muſt be granted from proving what they are alledg'd for: wherefore I will proceed to what follows.

Experiment III.

WE tried this Experiment again, and found a ſmall Bubble, much about $\frac{1}{8}$ of an Inch in Diameter, fill'd not only the Ball at the end of the Bolt-head (which was 1$\frac{1}{2}$ of an Inch in Diameter) but the whole Neck, which contain'd near as much Water as the Head, and beat down the ſurface of the Water within the Pipe much below that of the Water without the Pipe.

Theſe Experiments already found among my old Papers, will, I hope, without my ſeeking for more, ſuffice to manifeſt, that the expanſion which the Air may be reduc'd to without heat is indeed *Admirable*; for if we make an eſtimate of it but according to the Experiment which had the moſt moderate ſucceſs, it appear'd, that one ſpace poſſeſs'd, though not adæquately fill'd, by a portion of Air, may have its Air extended to at leaſt 2744 ſpaces equal to it; I ſay, *at leaſt*, becauſe very probably it was above twice as great: And if we make our eſtimate according to the moſt proſperous of our Trials, we muſt allow the Air to be rarefiable at leaſt 13000 times (I ſay again *at leaſt*) becauſe I am not ſure, that in that Tryal it was reduc'd (not fully, though perhaps very near) to the uttermoſt degree of rarefaction attainable in our Engin: ſo that I preſume you will now grant, that I ſpoke warily and much within Compaſs, when I mention'd but an expanſion from one to a thouſand.

And now having perform'd the promiſe I made you, it remains only that I take notice of the requeſt that you made me, about communicating theſe Experiments to the Curious. But this deſire of yours is oppos'd by no ſmall inconveniencies that would reſiſt my complyance with it. For it would oblige me by tearing out theſe Papers, to diſ-member a Collection long ago in making, and wherein they were plac'd to be much otherwiſe diſpos'd of, and not only make a greas gap in it, but ſtrip or deprive it of ſome things that were the likelieſt to re-commend it. Beſides, that theſe appearing before the reſt, are odd enough to make theſe ſeem far leſs uncommon than perhaps otherwiſe they would. Yet all this notwithſtanding, I find it uneaſie to refuſe what you and thoſe Friends that concur with you, on this occaſion, deſire, that if after having once more perus'd theſe Papers, you perſiſt in the ſame earneſtneſs you expreſs'd yeſterday (when you had not yet ſeen them) I ſhall not refuſe you the Diſpoſal of them, both for the Reaſon now given, and becauſe I have been inform'd as well by you as by other means, That the Rarefaction of the Air is at preſent the ſubject that buſies the Diſquiſitions of ſeveral eminent *Virtuoſi*, both Domeſtick and Forreign, to whom I pay ſo much reſpect, that I ſhall think it a happineſs, if it may be acceptable to them, not only becauſe it will be ſeaſonable, but becauſe, that though the Engin, that moſt of the Attempts were made in, has not been thought altogether Barren, yet theſe Trials will probably paſs for one of the leaſt Inconſiderable Productions of it: And theſe two Services I hope this ſhort Writing may do ſeveral Ingenious Readers; the one, that it will invite and accuſtom them to take notice of and conſider more than moſt, even of the moſt Learned, are wont to do, the great ſubtlety of Nature, and the ſcarce imaginable ſmallneſs of thoſe Aerial Inſtruments, that ſhe imploys even about viſible Operations: The other, that theſe Relations will excite the more Curious and piercing Wits to debate, and I hope help them to ſolve the two Problems here propos'd to them; *What* Figures and Motions may be aſſign'd to the Particles of the Air, to explicate its ſo wonderful Rarefiableneſs, and that perhaps without quite loſing its Durable Spring; and *How* the Air comes to be rarefiable ſo many times more *without* Heat, than hitherto we have found it to be by Heat. To which might be added, as a Third, *What* might be reaſonably conjectur'd about that part of the Cavity of an exactly cloſ'd Glaſs, Where, though the Eye diſcovers no viſible ſubſtance harbour'd in it, it appears not that the common Air does adæquately fill ſo much as the Ten thouſandth part?

NEW OBSERVATIONS

About the DURATION of the SPRING of EXPANDED AIR,

(Subjoyn'd by way of APPENDIX to the fore-going EPISTLE.)

FOraſmuch as re-viewing the former Paper about the *Rarefaction of the Air*, I took notice in the Cloſe of it of an Expreſſion (*viz. And that perhaps without quite loſing its Durable Spring*) which I fear may to ſome Readers ſeem to need Explication; it will not be improper on this occaſion to ſubjoyn ſomething by way of Appendix about it.

Firſt then, the Reaſon why in this ſhort Intimation I thought fit to imploy the diffident Term *perhaps*, was, becauſe really I had not (nor yet have) been taught by Trial, Whether and how far the utmoſt expanſion of the Air actually produc'd in my Engin, or otherwiſe procurable, and its retaining a ſenſible Spring, are conſiſtent? I expreſs my ſelf thus, to inſinuate, that I thought of other Inſtruments and Methods whereby the Dilatation of the Air may not improbably be meaſur'd and promoted; as by making the Torricellian Experiment in a Glaſs with a very capacious Head or Globulous part, and applying the Aerial Particles that will aſcend out of the ſubſiding Mercury, together with a Bubble of other Air, if it be needful to the uſe we have been ſpeaking. Something alſo may be done to ſome purpoſe with very fine and large Fiſh-bladders; but I ſhall not inſiſt on theſe or the other Expedients that came into my thoughts, contenting my ſelf to have intimated and thereby acknowledged, that there may be other means beſides the *Machina Boyliana*, to bring Air to a very great Expanſion. But whether any of them will ſurpaſs what has been actually attain'd in that Engin, time muſt declare; till when, we ſhall be content to make uſe of the Experiments it has already actually furniſht us with.

Wherefore to come to the ſecond or other remaining part of it, whereas in the mentioning of the Spring of the Expanded Air, I imploy'd the Attribute of *Durable*, you may eaſily gather the Reaſon from what I am now going to annex.

I had obſerv'd, not without ſome wonder, in the Inquirers into the Nature of the Air, that they have not, that I know of, ſo much as attempted to diſcover, Whether the Air either in the utmoſt or in the intermediate degrees we can bring it to, does retain a conſtant and durable Elaſticity?

For firſt, Men have not yet determin'd, Whether a portion of our common Air being exactly ſhut up in an Hermetically ſeal'd Glaſs, or ſome other exactly cloſ'd Veſſel, will conſtantly and uniformly, for a moderate time at leaſt, retain the degree of Elaſticity it had when 'twas ſhut up? And whether it will not ſometimes vary its preſſure, as we ſee that the Atmoſpherical (though I think upon peculiar grounds) is by the help of our Baroſcopes obſerv'd to do? Next it does not appear, Whether included Air, in caſe it retain an uniform Elaſticity for a moderate time, will retain it for a very long one? Nay, Whether it would not at length come not only to have a weaker Spring, but perhaps to have no ſenſible Spring at all? as we ſee it happen in Sword-Blades and divers other Springy Bodies, which after having ſtood too long bent, will continue ſo, and loſe their former power of Self-reſtitution, as they call it.

Thirdly, Men have not yet determin'd any thing about the degrees of the Airs Elaſticity, Whether the Durableneſs and Uniformity, or Varying of its Strength, may not depend upon the differing degree, it had when 'twas firſt ſhut up?

Fourthly, Much leſs have we yet attempted to diſcover, Whether the Spring of an incloſ'd portion of Air may be ſometimes weaken'd, and ſometimes ſtrengthen'd by the Changes, as to Gravity, of the utmoſt Atmoſpherical Air, the New and Full Moon? To which I might add divers other external Accidens, which as yet we ſcarce ſuſpect. And to theſe I might add ſome other Doubts and Inquiries, that may not be impertinently ſuggeſted, but here would, I fear, paſs for a Digreſſion.

Wherefore I ſhall proceed to tell you, that having taken notice of it, as an Omiſſion among the Inquirers into the nature of the Air, in whoſe negligence I was too long a ſharer, that we have not, that I know of, ſo much as attempted to diſcover this it ſelf: Whether the Air either in the utmoſt or in the intermediate Degrees of Rarefaction we might bring it to, would for a conſiderably long time retain its Elaſticity, or at leaſt ſome determinate degree of it, or loſe it by determinate and regular Decrements, I thought fit to make ſome Trials about this matter, but cannot brag of the ſucceſs of my Intentions, having been hindred either by want of Inſtruments, or by Removes, or by Sickneſs, or by unlucky Accidents, or by one unwelcome thing or other, from accompliſhing what I had chiefly deſigned, and partly alſo made ſome progreſs in it; but yet to give you ſome hints, as well as ſome occaſion to more proſperous Experiments, I ſhall not ſtick to annex what I readily call to mind about my attempts on that occaſion.

I remember then, that when I firſt began to try ſomething in order to my deſign, being deſtitute of fit Accommodations, I was fain to content my ſelf, by cauſing a good Bubble of Glaſs with a Stem to be ſo blown at the flame of a Lamp, that whilſt the Ball was yet exceeding hot, and conſequently contain'd none but highly rarefi'd Air, the Stem was very nimbly clapt into the flame of a Candle that was purpoſely kept ready at hand; ſo that being ſlender, it was in a trice ſeal'd up, and the Air within remain'd as much expanded as the great heat, it had been expos'd to, had brought it to be. This Bubble many Moneths after, I inverted into a Baſon of Water, and having broken off the Seal under the Surface of it, the Liquor was violently impell'd into the Cavity, but yet was not able to fill it, a conſiderable part being defended from the further aſcenſion of the Water by the Spring of the remaining Air, which for all the long ſtretch it had been put to, had not loſt any thing of its Spring that we could take notice of. But this was a Trial, in which I could by no means acquieſce; and therefore when I was a little more be-friended by opportunity, I try'd another way, partly to give a ſomewhat pleaſing ſurprize to unaccuſtom'd Beholders, and partly, becauſe though it could not ſhew all that I deſir'd, yet it might plainly ſhew, that the Air, even at a very conſiderable Extenſion, would hold out for a conſiderable time. Wherefore leaving a very ſmall proportion of Air in the Folds of a fine limber Bladder, whoſe Neck was very cloſely tied, I caus'd it to be, by the help of the *Ma-*

china Byliana, ſo expanded, that at length it ſo dilated it ſelf, as to ſeem to fill the whole Bladder, and reduce it to the extent it had juſt before 'twas empty'd; and the Bladder by a peculiar contrivance was ſo included in another Veſſel, that being protected from all intruſion of the outward Air, it maintain'd its plump and tumid Figure, and in that unwrinckled ſtate I ſhew'd it, *many Moneths ſince*, to ſome *Virtuoſi*, (now here in *London*) after it had continued ſo, if I miſtake not, near two year. Since the writing of this, I did at length find the newly mention'd Veſſel, and ſhew'd it to ſome curious Spectators, who with me took notice, that the included Bladder, inſtead of being wrinckled or ſhrunk, appear'd to be plump and full, as well blown Bladders are wont to be: ſo that *many Moneths* (perhaps a dozen) may be added to the freſhly mention'd Duration of the Expanded Air.

But this way ſatisfying me neither as to ſome of the Particulars I deſir'd my Attempts ſhould diſcover, I deviſ'd a little Inſtrument, whoſe Contrivance though it ſeem'd very ſimple, promis'd, and for ſome time gave me a far more accurate account of what I expected. The Inſtrument, if you deſire it, I can eaſily ſhew you, having lately been forc'd to make a new one, which is now by me; but it may ſuffice to tell you, that 'tis ſo fram'd; that 'tis fit to diſcover, beſides divers other things, Whether and how long Air brought to the greateſt Expanſion I could conveniently reduce it to in my Engin, will retain its Spring? And by what degrees or ſtages and periods of time, the decrement, if any be, is made? But of the iſſue of the Trial made in it, I can give you but a very imperfect Account, in regard that, though I made it about three years ago, yet having left the Inſtrument in a place where 'tis ſo lodg'd, that I cannot have it without returning thither, till I ſee it again my ſelf, I dare not venture to judge of the ſucceſs of the Experiment; only this I remember,

member, that I took no notice of any obſervable diminution in the Airs Elaſticity, though it were preſs'd, and, as it were, clogg'd with a weight, that one would wonder how it could, when 'twas ſo highly rareſy'd, ſupport for one minute *.

* See the Poſt-ſcript.

There is alſo another way that I contriv'd, wherein the Air in a little portable Inſtrument, which I can ſhew you, being expanded, as one may gueſs, to 5 or 600 times (perhaps 1000 times) its wonted extent, has not only for a long time preſerv'd its Spring, but ſatisfies me alſo about one of my chief Queries, which was, Whether the Air very much dilated without Heat, would be conſiderably ſenſible of external Heat? Which it plainly appears to be in this Inſtrument; where, notwithſtanding the great Rarity, it has already attain'd and ſeems likely to preſerve, the Heat of ones hand appli'd to the outſide (of the Veſſel) has a quick and very manifeſt Operation; and upon the with-drawing of it, the ſenſible Air quickly returns to its former Dimenſions, as well as temper; ſo that one may employ it as a kind of Weather-Glaſs, and perhaps make ſome diſcoveries by long comparing it therewith.

But hitherto I have been doing, what I do not love to do, and very rarely have done when I mention my own Experiments, that is, I have not punctually ſpecified any determinate quantities and proportions of the things ſpoken of; but one of my former Trials I have newly found out, regiſtred in a looſe Note, and therefore the Quantities being annex'd, I hope it may both give ſome countenance to what I have been ſaying, and give you ſome, though not an entire ſatisfaction about the thing it ſelf.

March 18. A Glaſs as Cylindrical as we could get it blown at our Lamp, and having a long Stem coming out at the unſeal'd end, was quite fill'd with Water and inverted into Water placed at the bottom of a large Pipe ſeal'd at one end, and of 3 or 4 foot in length. This external Pipe, ſo called for diſtinction ſake, was exhauſted, till the Air, that diſclos'd it ſelf in the Water of the internal Pipe, had drawn out the Water in the Cylindrical Pipe, as low as the upper part of the Stem; at which great Expanſion of the Air, the external Pipe being ſpeedily and ſecurely clos'd by a certain contrivance; the Air thus rarefi'd was kept ſometimes in my own Chamber that was warmer, ſometimes in an under-room; and after it had been kept from firſt to laſt about 11 Weeks or 3 Moneths, if I mis-remember not, without any other remarkable Variation, than that in the cold room, the Water aſcended, as I gueſs'd, about $\frac{1}{8}$, or near $\frac{1}{4}$, at that part of the internal Pipe where the lower end of the Cylinder gradually leſſen'd it ſelf into the ſlender Stem. Yeſterday I invited Doctor *Wallis* to be preſent at the breaking of the Glaſs, and to favour me with his aſſiſtance, for the better eſtimating the Expanſion of the Air upon the breaking of the clos'd Apex. The Water was but leaſurely (becauſe of the ſlenderneſs of the Orifice that was made for the Air to get into it) impell'd up into the formerly deſerted Cavity of the Cylinder, which it fill'd all, ſave a little Bubble which was exceeding ſhallow. We made uſe of our Eyes at a fit diſtance, and of Compaſſes both ordinary and Callaper, to obtain theſe meaſures. The Cylindrical part of the internal Pipe was 3 Inches in length and $\frac{3}{5}$ of an Inch (or leſs in Diameter on the outſide.) The Bubble was $\frac{2}{10}$ in Diameter and about 2 Centeſms in depth: From all which, by the Doctors Calculation, the Bubble, to the ſpace it poſſeſt unexpanded, was as 1 to 1350.

New Experiments
Touching the CONDENSATION Of the AIR by meer COLD,
And its Compreſſion without Mechanical Engins.

BEcauſe 'tis as truly as commonly ſaid, that *Contraria juxta ſe poſita magis eluceſcunt*, and becauſe what I am now going to interpoſe, is little leſs than neceſſary to be premiſed, to clear the way to what follows, and to connect the paſt writing to that which is to enſue; it will not be improper to add ſomething in this place touching the *Condenſation* and *Compreſſion* of the *Air*.

And here I cannot but a little wonder, that among ſo many, that have had occaſion to conſider the nature of Cold, and the Condenſation of the Air by it, I have not yet met with any that have had the curioſity to meaſure that Condenſation; wherefore I long ſince attempted to do it, as I have related in another diſcourſe; but not having that by me at preſent, and remembring in general, that I did it in Winter, when it may be objected, that the Air being already præ-affected with the coldneſs of the ſeaſon, was not capable of being ſo conſiderably contracted by an additional cold, as it would be at a time of year, when it is wont to be in a ſtate of greater laxity; I thought fit to make the Experiment about the beginning of *Autumn*, without tying my ſelf to make it with the ſame circumſtances that I had done before; the Event of this Trial I find regiſtred as follows.

[After the midſt of *September*, on a Sun-ſhiny day, and about Noon (which circumſtances we made choice of, that the Air might be the more rare and expanded) we took a Bolt-head or round Viol furniſhed with a long Stem, and plac'd it in a Frame purpoſely provided, ſo that the Stem was perpendicular to the Horizon, and the globous part was ſupported by ſuch a Veſſel, that thorow a hole, purpoſely made at its middle, the Shank reach'd downwards, till the Orifice of it was a little immers'd beneath the Surface of a Glaſs full of Water, that was plac'd at the bottom of the Frame. This done, we took a good proportion of Ice, and having beaten it in a Mortar, and mixt it with a due quantity of Bay-ſalt, we not only laid round about the lower part of the Ball, but the Veſſel, contiguous to that part, being purpoſely made with turnd-up brims, we were enabled to heap up the frigorifick Mixture, ſo as to bury the whole globulous part of the Glaſs in it, and cover the very top of it therewith to a conſiderable thickneſs; upon which occaſion the Air within being exceedingly refrigerated, the Water, into which the Shank terminated, was made to aſcend ſomewhat faſt along the Cavity of that Shank, till we perceiv'd it would reach no higher; but after a while began to ſubſide again; which nick of time being carefully watch'd, we made a Mark at the higheſt Station of the Water, and then taking out the Bolt-head, we fill'd it with Water, making an allowance for that ſmall part of the Stem which was immers'd at the beginning of the Operation. This Water we weighed, and found it amount to nineteen Ounces and ſix Drachms; then weighing as much Water, as ſuffic'd to fill the Shank up to the Mark newly mention'd, we found that to be one Ounce and three Drachms; by which number the former being divided, the Quotient was $14\frac{4}{11}$ Drachms; ſo that the proportion of the two quantities of Water being as 11 to 158, the ſpace into which the Air was condenſed by

Refrigeration, was to the ſpace it poſſeſs'd in its former ſtate of laxity as 147 to 158, and conſequently the greateſt Condenſation, that ſuch a time of the year and in ſuch Weather, ſo high a Refrigeration could bring the Air to, made it loſe but $\frac{11}{158}$ of its former extent.]

NB. Firſt, The Stem of the Glaſs ought to be of a conſiderable length, leaſt by the great contraction, made of the Air in the Ball by its high refrigeration, the Water ſhould aſcend into the Cavity of the Ball it ſelf, and thereby become exceeding difficult to be meaſur'd.

Secondly, If one would be nice, one may take notice, that the height to which the Water aſcended in the Stem, was about two foot; which Cylinder of Water, by its weight or tendency downward, might ſomewhat hinder the Liquor from aſcending quite ſo high as it would, and conſequently keep the Condenſation of the Air from appearing fully ſo great as it was, but ſo light a Cylinder as that of the ſuſpended Water, would ſcarce be very conſiderable.

Thirdly, When the Water was aſcended near as high in the Shank as it would riſe, there was obſerv'd in it an odd kind of *Subſultus*, or riſing and falling alternatively, almoſt like the *Mercury* in the *Torricellian Experiment*, before the *Mercury* comes to ſettle after its firſt ſubſidence. [But the conſideration of this *Phænomenon* belongs not to this place; for which reaſon I inſiſt not on this, and forbear mentioning ſome others.]

Fourthly, That though it appears not by this Experiment, whether the Cold thus produced is equal to that of Froſty Weather in Winter, and conſequently capable of contracting the Air as much as that ſeaſon is wont to do; yet by preceding Trials made with fit Inſtruments, I had found that by ſuch an application of Ice and Salt as we had made in the late Experiment, a greater degree of Cold, and that in a warmer ſeaſon, might be produced, than had been found neceſſary to make Froſty Weather in Winter: The way of Experimenting for brevities ſake I omit, but if you pleaſe you may command it.]

But 'tis not chiefly to acquaint you with the Condenſation that Nature uſes to make of Air, that I have been entertaining you with theſe Memorials; for that which makes it very pertinent to my preſent purpoſe is, that it will ſhew you, that as to the Condenſation or Compreſſion of Air that I am to recite, though Cold were emploied about it, yet it was not really produced by Cold, which could not contract the Air to ſo much as half that degree, you will find it was reduc'd to by our Operation (preſently to be mentioned;) wherein the frigorifick mixture did not primarily or immediately compreſs the included Air, but only ſo affected the Water that was ſhut up with it in the ſame Veſſel, as to make it ſwell, and conſequently crowd the Aerial Particles into leſs room: Wherefore it now remains that we proceed to the Experiment it ſelf; a ſhort Account of which, be pleas'd to take in the enſuing Tranſcript.

[To convince ſome Strangers, we took a new Glaſs Bolt-head, with a Neck not long, and fill'd it ſo far with Common-Water, that being Hermetically ſeal'd, the Liquor reach'd within three Inches of the Top, as near as we could gueſs, by meaſuring with a Ruler, and making an Eſtimate of the ſharp end, made ſo for the conveniency of ſealing up the Glaſs; which ſharp end we gueſs'd to be about $\frac{1}{4}$ of an Inch in length; then applying Snow and Salt to the lower part of the Bolt-head, we readily drove out the Water further and further into the Neck, till at length it was got up to the Baſis of the ſharp and conical end, where the Glaſs was ſeal'd; and then juſt as I was looking upon it, the Glaſs flew with a noiſe about my Ears, being broke into many pieces, which argued the Compreſſion of the Air to have been very great. And Doctor *Wallis*, who was preſent, and meaſur'd it from time to time, deſired me to regiſter the Experiment, with his Eſtimate of the Compreſſion, which was, that the Air was reduc'd into the 40th part of its former Extenſion.]

I know ſo great a Condenſation of Air will ſeem ſtrange to thoſe, that have taken notice, that ſome of the beſt Mathematicians of our Age, that have made uſe of Wind-Guns and other forcible Engins, to crowd the Air into as narrow room as poſſibly they could, confeſs themſelves not to have been able with all their ſtrength and induſtry to force the Air into leſs than the 15th part of its uſual extent; and beſides, that this was done in Countreys, where the Air may well be ſuppos'd more laxe and rare than in *England*. I confeſs, I ſaw no Trials made with Wind-Guns, that convinced me that the Condenſation was ſo great as that newly ſpecified: (about which *Merſen.* himſelf ſomewhat hæſitates, ſeeming to doubt, whether the Air were indeed reſtrain'd into a 15th, or but into one 8th part of its former room.) And he that hath obſerv'd and conſider'd, as I have done, that in Wind-Fountains (as they call them) of Glaſs, the Air will ſeem to be notably compreſs'd, whilſt indeed we could not find it compreſs'd into much leſs than its third part, will be the leſs unapt to be diffident of the great things that are ſaid of the Compreſſion of the Air; but becauſe Experience has informed us that our Engliſh Air may in peculiar Inſtruments be forcibly crowded into a 10th, 12th, or perhaps a 15th part of its former extent, I am content to take it for granted, what is related about the Compreſſion of the Air into the 15th part of its uſual dimenſions: and yet our Experiment will be a *conſiderabler* inſtance of the great compreſsibility, if I may ſo ſpeak, of the Air; for according to the

See Merſen. Phæn. Pneum. Prop. 32.

the Eſtimate delivered in the fore-going Narrative, or Compreſſion, which was without Mechanical Inſtruments or Engins, reduc'd the Air into the 40th part of the ſpace it had lately poſſeſs'd; and how great a force is requiſite, when the Air is once conſiderably condens'd, to ſurmount, though but a little, its great reſiſtance to further Condenſation, may be gathered from the Obſervations about the gradual renitency of the Air to Compreſſion, which we many years ſince made with *Mercury*, and afterwards publiſhed in another Treatiſe: but though, upon the recited grounds, that great Compreſſion of the Air produc'd by our Experiment, may, as I was ſaying, ſeem very *ſtrange*, yet it would not ſeem *incredible*, if I ſhould here borrow thoſe Experiments and Obſervations from my already publiſh'd *Hiſtory*, and ſome *unpubliſht Papers* about *Cold*, that would countenance what I have been delivering; and eſpecially if I ſhould ſtay to communicate to you the way, I not unſucceſsfully made uſe of, to eſtimate by *weight* the great force of the expanſion of Water upon its freezing. But ſince an account of this contrivance is not here neceſſary, and would require more leaſure than I can ſpare at this time; it remains only, that by way of Corollaries from what has been hitherto delivered in this and the two precedent Writings, we rather point at, than diſcourſe of, ſome Obſervations that it ſuggeſted to us, in the enſuing Paper.

Defence againſt Linus.

Of the Admirably Differing EXTENSION Of the ſame Quantity of AIR RAREFIED and COMPRESSED.

HAving already declared, that what I pretend to in the Cloſe, is but to ſet down ſome Obſervations, that reſult from, or are ſuggeſted by what hath been already delivered; I preſume I need not trouble you or my ſelf, with any other *Preface* to what follows.

That then which ſeems firſt worth taking notice of, is the differing alterations that the Air is ſubjected to by Cold and Heat; For whereas we could not find in this our Climate, that Cold would reduce the Air into near the 20th part of its former Extenſion by Condenſation; Heat would advance it to near 70 times its uſual Laxity by Rarefaction.

Next, We may obſerve, That by Engins and other Artificial Inſtruments, the Air may be two or three times as much compreſs'd, as Nature is wont to condenſe it by Cold, even in Froſty Weather; and ſo on the other ſide, the Air may by the intervention of Art and Inſtruments be much more rarefied and expanded, than it has been yet found to be by the bare application of External Heat, though it were that of an intenſe Fire it ſelf.

Furthermore, It may ſeem worth while, to obſerve, how much the utmoſt degree of Rarefaction by Heat, that Experiment has ſhewed us of the Air, falls ſhort of the Degree of Expanſion, to which it has been advanced in our Pneumatical Engine, the proportion betwixt theſe two Expanſions being that of 1 to 70, or thereabout.

See, as to the utmoſt Expanſion by Heat, Merſenn. in Cog. Phyſ. Mathem.

But perhaps it will not be neceſſary to conclude, That the Air is ſo much more rarefiable than compreſsible, as moſt Readers will be prone to infer, by comparing the greateſt Compreſsion and Expanſion of it, that are mentioned in theſe Experiments; ſince, if I miſtake not, it ought to be conſidered, that the Air, we made our Trials with, upon the ſurface of the Earth, was not (no more than is the Air we commonly breath) properly in a true natural Conſiſtence as they ſpeak, or, if you pleaſe, in a free and indifferent ſtate in reference to Rarefaction and Condenſation, but was already highly compreſs'd by the weight of the Atmoſpherical Pillar that lean'd upon it: ſo that it had already a very ſtrong renitency to further Compreſsion; whereas the Air that was to be rarefied, had by vertue of its Spring (ſtrongly bent by the weight of the Incumbent Air) a ſtrong Propenſion or tendency to dilate it ſelf; which difference I muſt content my ſelf to have intimated, and leave you to conſider, Whether and how much it may alter the Caſe.

Fourthly, To ſome perhaps it wi'l ſeem more fit to conſider, than eaſie to reſolve, how, ſince the Corpuſcles of the Air are acknowledg'd to be heavy, and thoſe that remain, muſt be ſo wonderful thinly diſperſt, in the Cavity of the Receiver they come to be ſupported, and kept as it were ſwimming therein, and do not appear to ſubſide by their own weight, the *Materia ſubtilis* (though the preſence of that ſhould be admitted) not appearing to have gravity, wherewith to ſuſtain them; and the *Vacuum* (if that be ſuppos'd wherever the Aerial Particles are not) being too near a-kinne to nothing, to be able to oppoſe their deſcent: but though ſomething may be ſuggeſted about the ſolution of this doubt, my haſte obliges me to leave it ſuch.

Fifthly, I will not make it my buſineſs to make mention, in this place, of the wonder that may be *juſtly* excited in thoſe, that, when they look on one of our well exhauſted Receivers, attentively conſider, how ſmall a proportion, the common Aerial Corpuſcles, which are very ſparingly diſpers'd there, bear to the whole Cavity of the Veſſel, which, before it was exhauſted, was thought to be repleniſht with Air alone. This I ſay, I ſhall not ſollicitouſly obſerve, becauſe I think I need not; for I little doubt, the thing will be obſerv'd and laid hold off, both by the *Carteſians* and *Epicureans*; the *former* of which will endeavour thereby to eſtabliſh the neceſſity of their *Materia ſubtilis*, to maintain the Plenitude of the World, and the Circle they attribute to Moving Bodies; and the *later* will here triumphantly pretend to have a more illuſtrious inſtance, than ever, of their *Vacuum coacervatum* within the World, ſince here is an impenetrable Veſſel, out of which 'tis manifeſt that an almoſt incredible proportion of Aerial ſubſtance hath been manifeſtly made to iſſue; whereas 'tis no wayes manifeſt to any of our Senſes, that any other body has got in to ſucceed in its room: Wherefore leaving them to debate, what it is that is contain'd in that far greateſt part of the Veſſel, that the Air pumpt out of our Receiver has deſerted, I take notice,

Sixthly, That, to conclude with what was the main drift of this and the fore-going Papers, we are here invited to obſerve with wonder the ſtupendious mutability of the Air, as to rarity and denſity, whereby the ſame quantity of Air being ſometimes compreſs'd, ſometimes dilated, may change its dimenſions to a degree, that ſeems almoſt to tranſcend the power of Nature and Art; and by conſequence might probably be rejected as incredible, if it were abruptly and nakedly propos'd: and therefore it will be convenient to do, though very briefly, theſe two things:

Firſt, To conſider, what we have upon experience deliver'd in our *Defence againſt the Learn'd* Linus, touching the Condenſation and Rarefaction of the Air, as 'tis expoſed to a greater or ſmaller preſſure, without the intervention of either External Heat, or elaborate Engins. For from theſe Experiments (that may be found in the lately mention'd *Defence**) Eminent Mathematicians have inferr'd, that one can ſcarce ſafely put determinate limits to the ſtupendious Rarity, which the upper part of the Atmoſphere, being almoſt totally un-compreſs'd by incumbent Particles of Air, may be ſuppoſed to have by Nature, un-aſſiſted by Art.

And this is the firſt of the two things, I above deſired to have taken notice of. But the *other* (which though it be but the ſecond, is much the more conſiderable) is, to conferre together the ſmalleſt extent, to which we have reduc'd it by Condenſation, and the greateſt, to which we have advanc'd it by Rarefaction, after having taken notice, that, according to the leaſt eſtimate of any recited in the fore-going Experiments, the extenſion of the ſame quantity of Air, is as 1 to 2744, or thereabouts, and if, inſtead of the moderateſt, we take the greateſt Expanſion of the Air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncompreſt Air was highly rarefied, that number being multiplied by 40, becauſe of the fore-mentioned compreſsion of the Air, will amount to 520000, for the number of times, by which the Air at one time exceeds the ſame portion of Air at another time: which is a difference of Expanſion ſo great, that I hope it will keep you from thinking the Title of the fore-going Epiſtle, where the Expanſion of the Air is called *admirable*, immodeſt; eſpecially ſince I have forborn to mention, what probable arguments might be offer'd, to prove it at leaſt poſsible, that the induſtry of men, and perhaps our own, may find means, to make both the Condenſation and Rarefaction of the Air to exceed the uttermoſt, whereto we have yet been able to bring them.

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* Chap. 5. whoſe Title is, *Two New Experiments touching the Meaſure of the Force of the Spring of the Air compreſs'd and dilated.*

POSTSCRIPT.

Touching an Obſervation to be inſerted above, Pag. 16. *immediately after the Mark* *.

Since the writing of this, the Author chanced to find one of the lately mentioned Inſtruments, of a conſiderable bigneſs, which was preſumed to have miſcarried; and comparing it with a Memorial made, when 'twas firſt compleated, to keep in memory the heights, dimenſions, &c. of the incloſed *Mercury* and *Air*; we found, that in about 10 weeks there was not any conſiderable variation of them; and the little ſhrinking of the Air, which was diſcoverable by an attentive Eye, was not ſuch but that it might be probably aſcribed to the change of the weather to a far greater coldneſs, which might be ſuppoſed, a little (and it did it but very little) to weaken the Spring of the included Air, and conſequently abate of its full reſiſtance to the preſſure of the *Mercury* in the longer leg of the Syphon.

FINIS.